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ON COLLECTIVE PHENOMENA

AND THE

Scientific Value of Statistical Data

BY

ERNEST G. F. GRYZANOVSKI

With an Introduction by FREDERICK TUCKERMAN

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THE PRINCETON UNIVERSITY PRESS
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INTRODUCTION.

Ernst G. F. Gryzanovski (otherwise Grysanowski or Grisanowski), the author of this essay, was a man of very remarkable attainments, and of very brilliant qualities as a writer. Born May 10, 1824, at Königsberg, he entered the university there, and in 1845 took his degree, with high honors, as Doctor of Philosophy, after an examination in mathematics and Oriental languages. He was also a deep student of the Hegelian philosophy. In 1847 he was appointed attaché to the Prussian Embassy at Rome, and there acted as secretary to the Ambassador von Usedom. Throughout the eventful period of Italian history that followed, he remained in this capacity, and his letters to his family describe vividly and graphically the events of those important days. In April, 1849, not many months after the flight of the Pope to Gaeta, he retired from the diplomatic service, having become disgusted with diplomacy. After a residence of two or three years, partly at Rome, partly at Genzano, supporting himself by teaching mathematics, Gryzanovski decided to follow a taste which had always been strong with him, and began the study of medicine. He studied first at Pisa, and subsequently at Naples, Rome, Montpellier, and Bonn, taking his degree of M.D., *insigni cum laude*, at Heidelberg in 1855. He practiced his profession at different periods in Florence, Pisa, and Leghorn. His residence at Florence was for him a very agreeable one, as he was on terms of intimate friendship with Walter Savage Landor, the Brownings, the Trollopes, Mrs. Somerville, and many other celebrities then residing there.

At this time he wrote much on social and hygienic questions. Somewhat later he became deeply interested in efforts made by the humane societies in England and Germany to restrain the practice of vivisection, and did admirable and valuable work with his pen in behalf of suffering animals. In 1871 he was offered the professorship of German at Harvard College, which, however, he declined. From 1869 to 1872 he corresponded regularly with the *Nation*, and it was the opinion of the editor at that time that no foreigner in his experience ever used the English language so correctly and gracefully as he. From 1871 to 1877 he wrote many articles for the *North American Review*, and Mr. Adams, the editor, used to say that Dr. Gryzanovski was his best contributor. " His English," says a writer in the *Athenaeum*, was incomparably the purest we have ever known a foreigner to use." His subjects show a remarkable variety and range, *e. g.*, " The Origin and Growth of Public Opinion in Prussia," " Regeneration of Italy," " International Workingmen's Association," " Schopenhauer's Pessimistic Philosophy," " New Trials of the Roman Church," "Comtism," "Wagner's Theories of Music." Dr. Gryzanovski died at his villa in Segromigno, near Lucca, May 31, 1888.

FREDERICK TUCKERMAN.

Amherst, Mass.,
June 18, 1906.

ON COLLECTIVE PHENOMENA AND THE SCIENTIFIC VALUE OF STATISTICAL DATA.

BY THE LATE DR. GRYZANOVSKI.

There is a strange fascination in numbers. Not only the mathematician, and the mystic philosopher, but the artist, the physicist, the economist, all feel it alike, and even those who are unable to comprehend the real nature of numbers, have an instinctive appreciation of their conclusiveness. The reasons of this are by no means generally understood, and the success with which numbers are used in lieu of arguments is greatest where those by whom or against whom they are used are unconscious of these reasons. Numbers are the artillery of controversy; they overawe the opponent, and, like artillery, they are surrounded with a certain halo of mathematical positiveness which we are far from wishing to destroy, but which ought not to be magnified by ignorance and timidity. Unquestionably, the statistical method is a precious tool, but it is also a very delicate one which, when blunted by unskilled handling, may spoil the work for which it was designed.

An example will illustrate this. During the recent agitation against compulsory vaccination in Germany, the learned Professor Kussmaul took pains to find out that 3330 cases of smallpox occurred in Marseilles in 1828, and that of these 3330 persons 2289 had not been vaccinated. Of these latter, 420 or 18.3 per cent. died, of the 1041 vaccinated ones only 17 or 1.7 per cent. died. The data being presumably correct and the calculation

obviously faultless, the efficacy of vaccination seems proved. But now Dr. Lorinser comes and tells us that in 1828 the population of Marseilles was 133,000 of whom 33,000 were vaccinated while 100,000 were not. And if of the former 1041 or 32 per mille caught the disease and of the latter 2289 or 23 per mille, this not only disproved the protective power of vaccination but proves its noxiousness. Which of these pleaders is right? Both may be wrong in their conclusions, but the statistical premises of Dr. Lorinser are more logical than those of Prof. Kussmaul. Many people will see this on comparing the two, but few would have detected the flaw in Prof. Kussmaul's unchallenged argument.

Now considering the manifold usefulness of the statistical method in almost every field of scientific enquiry, and considering how often and how easily the general public is misled by amateur statisticians, it will be admitted that the elements of statistical philosophy deserve greater attention than they have hitherto obtained. Even as a weapon of defence, they are not to be despised, and dry though the subject may appear at first sight, nobody will regret having bestowed a little trouble on its study.

Triumphant opponents often urge that "facts speak for themselves." No doubt they do, and so do the mouths of cannons. Yet eloquence is no exclusive prerogative of facts, considering that any logical or moral axiom speaks not only as loudly as any fact, but the more loudly for not being a fact. Facts can say and do say to us only these three things:

"We are what we appear, or are reported, to be,
"We are actual effects of more or less inferable causes,
"We must or may occur again under similar circumstances."

Each fact, therefore, presents itself under three possible aspects: as a statement, as an effect suggestive of causes

or of cognate effects, and as a future possibility. Only in these three relations, facts can become interesting, and in accordance with these three relations, statistics must have a threefold task: they must begin with the *registration* of facts, then pass on to their *causal interpretation* or to the estimate of their retrospective certainty and end with an estimate of the *probability* or prospective certainty.

But before examining the nature of these functions we shall have to examine the nature of the materials used in statistics. Pure numerals are meaningless; their value lies in their relation to some unit which may be either a generic term or some arbitrarily chosen plurality of specials.Statistical data, therefore, are in reality *ratios*, not numbers. In a list of percentages we may omit the common denominator, but a parliamentary majority which is a numerical difference, can have no meaning without the addition of the whole number of voters. A ratio is registered, not as a *semel factum* that might be interesting for its own sake, but as a first specimen of a whole class of possible facts for which we are willing to wait. These successive ratios, or rather their numerators, may be equal or different: in either case it is not these numerators which interest us but their *variableness* or *invariableness*. In other words, if the primary materials are numbers and ratios, its secondary materials are *laws* and causal *connexions,* for we cannot witness repetition, sameness, change, periodicity, without looking for its cause or for its law.

If the variations are quite irregular and their range very wide, we may well despair of finding their causal conditions, but if they are regular, showing either a steady increase or a steady decrease or something like periodicity, the law or agency that regulates these changes will generally be discoverable. More remarkable, however, than any change or periodicity will be the constant re-

occurrence of the same ratio. Such ratios are called *Nature's Constants,* and most of them belong to mathematics, physics, chemistry, astronomy. Yet statisticians are always eager to find new ones and that, too, in spheres where evolution reigns supreme and where evolutionists ought to be the very last persons to seek for treasures.

These constants of nature speak like all other facts, but they only say: we are what we are and what we ever were, parts of the eternal fitness of things on whose why and wherefore it is useless to speculate. They are stubborn, sterile facts, striking yet not intoxicating, because their immutability hides their causes and places them altogether beyond our reach. A point may be part of a line, but if we only see the point, we cannot trace the line on which it lies, *unless it moves in it,* and a fact which may be a link in many causal chains cannot betray to us its real causal chain, unless it is capable of undergoing some changes be they ever so small. If the fourth decimal in the number π were 6 instead of 5, if falling bodies traversed 20 feet instead of 15 in the first second, or if the atomic weight of carbon were 77 instead of 75: we should accept these data without being shocked or embarrassed by them. They would upset no doctrine, disturb no habit of thought. Their fixedness isolates them. Nature's constants are Nature's alphabet and as such must be learnt, but even if we knew them all, we should be as far as ever from knowing Nature's grammar. Even in biology which is the science of evolution or everlasting change, such constants have been sought for, but the results cannot claim more than an apparent or relative friendship, and the value attached to them is not scientific but practical. If we ask, for instance: what is the normal length of the human life? it is scientifically indifferent whether the answer is "three-score and ten" or 72 or 80 or any other number. No number, be it ever so accurate, would help us understand why the

organic machine which constitutes our body was wound up for that particular length of time, and how it comes that a contrivance, after having supported and readjusted itself for so many years, ceases to do so at that age rather than at any other. This kind of physiological insight can never be furthered by biological statistics. If, nevertheless, we care to know and take pains to ascertain, whether the psalmist's estimate holds still good in our days, our motives are chiefly practical. Our life assurances and the general range of our worldly hopes and aspirations depend on the result of that enquiry, and only when these results show certain variations, when the "normal" length of life is found to be 74 in one country, 70 in another and 67 in a third, the facts regain their proverbial eloquence and become scientifically interesting. As one of Nature's constants one number is as good as another; as Nature's variables these numbers become scientific problems. For, although constancy must have its causes as well as variableness, the causes of constancy are inscrutable while the causes of variableness and the law or rate of the variation itself are either calculable or inferable from the variations observed.

A pursuit which consists in registering facts, inferring causal connexions and estimating probabilities must naturally have a wide range of activity. Yet statistics claim more than their due when they refuse to acknowledge any limit to their competency. They pretend to be, like philosophy, a universal science, a solvent for all problems whether relating to ethics or physics, to sex or health, to trade or morality. And this indiscriminate application of one method of enquiry has naturally led, and continues to lead, to many fallacies, not to speak of the logical error of the application itself. If we examine more closely the apparently boundless area of statistical enquiry, we soon discover certain lines of demarcation by no means coinciding with the boundary lines of the different sciences

but dividing the whole area into three broad sections of different degrees of scientific dignity or positiveness. This division being independent of the subject matter, no single science lies entirely in one of these sections or can fill up the whole of any such section, so that each section will embrace parts of several sciences, and within each section the statistical method will yield results of the same scientific validity. Statistics pretend to reign supreme in all these sections, but we will endeavor to prove that, while their *authority must* be fully acknowledged in one of them, and while their *services may* be accepted in another, neither their rule nor their services are admissible in the third. In other terms: we shall prove that statistics are within certain limits, a genuine and *independent science,* but that, on going beyond these limits, they become, on one side, an *auxiliary science,* a mere method, and, on the other, a trespasses.

These three sections are the realm of necessity, the sphere of probability, the region of incalculableness. We comprehend a phenomenon when we know all its causes, and this knowledge may be perfect, incomplete or impossible. If it is perfect, two cases are possible: the causal connexion may be *a priori* intelligible, so that the mathematical or logical certainty we already have, could not and need not be enhanced by further observation and computation;—or we may know empirically that the phenomenon never fails to occur when certain causes coöperate and never does occur when only some of these causes are at work, and until this experience is acquired, repetition of observation or experiment is, of course, necessary. In either case we have, or obtain, an adequate knowledge of causes and necessities, and a phenomenon whose causes are understood must be, *pro tanto,* predictable, its prospective certainty being as great as its retrospective necessity.

But if we know only some of the causes necessary to

produce the phenomenon, the re-occurrence of this group of causes will be more frequent than the re-occurrence of the phenomenon which, consequently, cannot be predicted with certainty. And, lastly, if we know none of the causes or, which is practically the same, if the number of possible causes is so great that we cannot grasp the intricacy of causal connexion,—in other words, if the phenomenon is the result either of arbitrary volition or of so-called accident: both our comprehension and our prescience will be nil and we must content ourselves with being the describers or historians of the phenomenon.

Thus we obtain (to change our metaphor) three distinct *levels* of enquiry: the level of adequate comprehension, the level of partial or imperfect comprehension, and the level of historic knowledge. Or we may call them: the level of necessity and certainty, the level of contingency and probability, and the level of freedom and "accident." On the first level we have problems and adequate data for their solution, on the second we have problems but no adequate data, each datum implying a problem and each problem being only imperfectly soluble, and on the third level we have to deal with results which, if considered as problems, are insoluble, but if considered as matters of fact, are fit materials for description or historic record.

Now it is obvious that on the first level statistics can never be more than an auxiliary method. Where we have *a priori* certainty, one fact is as good and as conclusive as a thousand, although a small number of repetitions may serve didactic purposes by exemplifying what may require illustration, not by proving what requires no proof. And where our knowledge is only empirical, the statistical treatment may give it any desirable degree of accurracy but cannot enrich it by new principles or points of view.

On the second level, on the contrary, where imperfect

comprehension of the causal connexion forces us to substitute theoretically possible causes for real causes and probability for certainty, the statistical method becomes indispensable, because by multiplying the data, it reduces the number of theoretically possible causes. The real causes being contained in the possible causes, a reduction of the latter must lead to an approximate knowledge of the former.

Not so on the third level. The phenomenon wrought by free volition and by so-called accident are effects of a causation of transcendent complexity. We know nothing about this causation, or the words 'freedom' and 'accident' would never have been coined. We can no longer determine the theoretically possible causes, and even if by conjecture we had found some of them, their number would no longer comprise all the real causes, as in the former case. The only thing we know of these real causes is, that their number must be very large. Therefore, what was the wider conception in the preceding case, becomes here the narrower one, and if statistics by multiplying the data, reduce the number of theoretically possible causes, that is to say, make the narrower conception still narrower, the result must not be an approximation to the knowledge of the real causes, but rather a retreat from it.

If, then, statistics appear as a useful, though by no means indispensable *auxiliary* on the first level, and if they cannot pretend to be more than a descriptive and recording agency when intruding on the third, they certainly have a right to consider the second level as their proper domain and to apply their rules and methods to all problems and inquiries that can be proved to belong to that level. Nor will it appear strange now, if the scientific dignity of statistics is found to begin where scientific positiveness ends and if it proves greatest where certainty and uncertainty are most evenly balanced.

Having thus defined the legitimate sphere of scientific statistics, we return to the consideration of their functions and operations. These functions, as we saw above, consist in the registering of facts, in the inferring of their causes and connexions and in the estimating of their probabilities.

I. REGISTRATION implies *classification, counting* or measuring, *averaging* and *tabulating*. Facts must be classified according to what they have in common, that is to say, either according to their outward appearance or according to their causes. But community of cause being generally an open question at this stage of the inquiry, it will be prudent to begin with groups formed according to outward similarity. The general death-rate of each locality and for each period of life must be known, before the mortality due to any particular disease or epidemic can be properly investigated.

Only equals or similars can be counted. By counting them we efface their individuality and merge quality in quantity. Thus numbers become a solvent which may again be eliminated when it has done its duty, just as a chemical solvent is allowed to evaporate when it has served its purpose, which was to reveal hidden affinities or to transform shapeless masses into well-defined crystals.

But the counting of one series of facts only corresponds to the measuring of one value. However numerous these facts, their counting constitutes but one observation, and repetition being the essence of statistics, we must wait for, or artificially obtain, a new series of similar facts and as many of such series as the nature of the problem requires. If these groups are sufficiently similar. differing only in the time or data of their occurrence, we shall obtain a series of sums or numerical values referring to the same unit and therefore comparable. And here

two cases are possible. Either these values form an altogether irregular succession of sudden or gradual increments and decrements, or they exhibit a certain *prima facie* regularity, grouping themselves with a certain evenness of distribution on either side of an ideal medium value.

This ideal value is called the *average* and the most generally accepted form of average is the arithmetic mean, which is the sum of the item divided by their number. It may appear arbitrary to substitute such a formula for a plurality of actual data very few of which, if any, correspond exactly to that formula. Nor is the arithmetic mean, as we shall see presently, the only possible form of average. But mathematicians know that it has the important advantage of being deducible from a more general law called the rule of minimum squares which is used by physicists, astronomers and chemists for the correction of " personal " and accidental errors of observation. And since this rule rests on the assumption that there is but one true value and that the discrepancies of the observed data are due to human fallibility and to other accidental perturbations which are not the essential factors of the phenomenon examined, it follows that the arithmetic mean may be used as the one true value, that is to say, may be substituted for the many discrepant values observed, in all cases where the width of their discrepancies is sufficiently small and their distribution on either side sufficiently even to warrant the assumption that there is but one true value. Of the two conditions, however, the smallness of discrepancy is far less essential than the evenness of distribution. How wide these discrepancies or the *statistical dispersion,* as it is called, may be without excluding the use of the arithmetic mean, is shown by the isothermal lines which connect places of the same mean temperature. London and China, for instance, lie on the isotherm of 50°, but the number of days with a tempera-

ture of *about* 50° is very large in London and almost nil in China, and it may be questioned whether a place where only the extremes of cold and heat prevail, can be said to have a mean temperature at all. Such a mean is always calculable but, unless the "dispersion" is small, has a purely ideal significance. It will be better in such cases to reduce the unit by subdividing the series of data into portions sufficiently small to exhibit a smaller dispersion. By substituting the twelve monthly averages for the yearly average, we obtain results at once more plausible and more actually true. We refuse to believe that China has "the same temperature" as London, the two places being something like climatic opposites, but we readily admit that China has the same midwinter temperature as the North Cape and the same midsummer temperature as Morocco, the monthly isotherms expressing truer or less ideal equations than the yearly ones.

The great objection, then, to the use of the arithmetic mean in cases of wide statistical dispersion lies in this, that not one of the observed data may be equal or nearly equal to the mean value found by calculation. And this may have induced men like Fechner and Galton to search for other methods of averaging. To Fechner we are, indeed, indebted for two new forms of average which he has called *Centralwerth* and *Dichester Werth* and which we propose to call *central* or *ordinal mean* and *frequential mean* respectively. The ordinal mean is obtained by arranging the observed data according to the numerical value, by then counting them and taking the central item which is separated from the largest and from the smallest by an equal *number* of intermediate items. This mean has the advantage of being itself one of the data observed, so that whatsoever can be predicted of the data can be predicted of their central mean as one of these data, which is not the case with the arithmetic mean. Cournot, though not knowing the 'central' average, has furnished

us a most instructive example for this fact, by pointing out that when on a final hypothenuse we construct a number of rectangular triangles, and then take the arithmetic mean of each of the two sides, these two mean lengths will *not* form a rectangular triangle with the given hypothenuse. Here, then, is a case where the arithmetic mean cannot logically be used as a representative or abridged substitute for the whole group of data, whereas the central means of the two sides, occurring as they do among the triangles actually constructed, might, if any summarizing were required, be used for that purpose.

The other form of average proposed by Fechner is determined by the *frequency* of occurrence. If we arrange the data according to their value or quantity and then divide this series into equal portions, a certain unevenness of distribution will be noticeable; if we then take the most crowded portion and from it its central item, we shall obtain a value which, in certain cases, may be used as the representative of the whole series, it being the one which actually occurs more frequently than any other.

The difference between these three forms of average may be thus briefly defined: in the arithmetic mean the *sum* of the positive deviation is equal to the sum of the negative ones and the sum of the squares of all the deviations is a minimum;—in the ordinal or central mean the *number* of the positive deviations is equal to the number of the negative deviations, and the sum of all the deviations is a minimum;—and in the frequential mean the number of deviations is greatest where their sum is smallest.

If, for instance, we wish to determine the mean age of a dead generation, we are free to choose between these three forms of average, but the three results will have different meanings and different degrees of importance. The arithmetic mean will give the age which every individual would have reached without exceeding it, if life

were evenly distributed among men. Such an age is nothing but a calculated abstract and may in reality be the one more frequently surpassed or not reached than reached and not surpassed. But both the ordinal mean and the frequential mean will be realities, the ordinal mean giving us, in this case, the age which has been as often surpassed as not reached, and the frequential mean representing the age which was actually the one most frequently met in that generation. It is the last two numbers (and more particularly the central mean) which interest insurance companies, while the arithmetical mean seems to have no practical value in this case.

Of course, the three centres may coincide, or two of them may, but the central coincides more frequently with the arithmetic mean than the frequential mean with either of them. In all such cases of coincidence or even of quasi-coincidence we may assume that we have to do with phenomena of a considerable degree of fixedness, whose averages have been called *typical* by Quételet. A 'typical mean' is, in fact, for biological and social phenomena what Nature's constants are for physical phenomena.

We began by saying that the successive results of statistical computation may either show a certain symmetry of deviation from an ideal mean value, or have an altogether irregular appearance. We know how to deal with the former case, but what are we to do with the latter? We may divide a series of irregular data into sections and calculate the average of each, but what is the use of a series of averages which must be as irregular as the original data? No doubt we may discover regularity of change instead of a fixed value, but very often we only have the change without a trace of regularity, and until we can decide whether the change is regular or lawless, we must resort either to *tabular* or to *graphic registration*. These two modes of registering are logically the same, the two heads or entries of a statistical table

being to the list of data what the abscissas and ordinates are to a geometrical figure of two dimensions. Both methods are the reverse of the method of averaging, for while the latter consists in condensing and summing up, the former two not only leave the series of data unabridged but often enrich it by *interpolation*. The graphic method, especially, is essentially interpolation, the image of tabulated data being always a dotted line which has to be rounded into a *curve*. By rounding off this line we, no doubt, commit an infinite number of infinitesimal errors, but these errors are not greater, they are indeed. smaller than the errors of omission we commit in using one mean value in lieu of a multitude of data. The specialization implied in the average is generally bolder than the generalization implied in the statistical curve. Both are useful operations, because the former enables us to speak of a multitude without enumeration, the latter to see or to conceive a multitude as a continuous whole. The average is its numerical *name*, the curve its intellectual *portrait*. The name not being a definition and resting on omission, is always below par or *sub-adequate* (if we may coin such a term), while the graphic image, implying interpolation, is always above par or *super-adequate*. There is an ideal ingredient in both.

II. INTERPRETATION of statistical data. The registered data being either fixed values and ratios or a sequence of variable values, the thing to be interpreted can only be the fixedness or variableness of these values, but all registration rests on the assumption that the phenomena to be counted as one collective phenomena are held together by some conceptional bond of union, and the minimum of this connectedness is outward similarity.

A group of phenomena sufficiently similar to be counted, may be the result of the joint action of many dissimilar causes. If among, or besides, these causes there is one common to all, the lines of causations may be

said to *converge* towards this common cause; if all the causes are different, these lines of causation may be said to *diverge.* Now it is obvious that community of cause is as strong a bond of union as common descent or relationship, but community of effect as a bond of union between these effects means nothing but similarity. It may be a bond of union between the causes, especially when these causes are free agents and their coöperation is intentional, (so that community of effect becomes community of motive which means community of cause in the case). But the phenomena whose connectedness we have to deal with, are the effects and not the causes or agents that wrought them. We may say, therefore, that whenever many dissimilar causes unconsciously coöperate in producing a group of similar effects, this similarity does not constitute relationship but must be considered as *accidental,* if by accident we understand the coöperation of causes either unknown or too numerous to be reckoned with.

Thus we obtain two great categories of collective phenomena: the *connected* and the *unconnected* phenomena (the latter being connected only by outward similarity). But as we have, between convergence and divergence, the case of parallelism and (to be quite exact) the case of asymptotic convergence, we have to admit here, too, an intermediate class of phenomena whose causal lines point towards some unknowable common cause lying at infinite distance. To search for this cause would be a waste of labor; if, nevertheless, we like to go back along such groups of parallel lines, we do so in hopes of finding some other equally collective phenomena which, in this causal pedigree, is anterior or ancestral to the given phenomenon from which we started. This ancestral phenomenon may be known to us by previous experience, or it may be a theoretical inference, a presumptive predecessor; in the former case we may consider the given phenomenon as

its *probable effect,* in the latter case, as its *probable symptom.* We say 'probable,' because there is no absolute certainty in these complex causations, and there can be no absolute certainty, first because the ideal cause or point lying at infinite distance, may lie on either side of our given phenomenon, so that we cannot always tell which of the two phenomena is cause and which effect; and secondly because if certain causes are known to produce, severally, certain effects, it does not follow that the sum of these causes when acting together must produce neither more nor less than the sum of those effects. The neglect of this interdependence or interference of parallel phenomena is one of the most ordinary sources of statistical error.

In 1868 (to quote a well known instance) the Registrar-General of Scotland, comparing the death-rate among single persons with the death-rate of married persons of the same age, came to the startling conclusion that bachelorhood was "more destructive of life than the most unwholesome trades or the residence in an unwholesome house or district where there has never been the most distant attempt at sanitary improvements of any kind." The fallacy of this conclusion was promptly pointed out first by Mr. Proctor in the Daily News of October 17th, 1868, and later by Mr. Darwin in his "Descent of Man" (I, 176), and as neither of these authors impugns the accuracy of the data, the fault must lie in their interpretation. Matrimony may be, directly or indirectly, conducive to health and longevity, but at the same time, there is "a principle of selection which tends to fill the number of married men from among the healthier and stronger portions of mankind." Each of these two phenomena, matrimony and health, may be cause or effect to the other, though probably not in the same degree.

The greater the convergence of the causal lines, the less uncertain will be our interpretation. When we examine the insalubrity of trades, for instance, no invasion of cause and effect is possible. Health and infirmity may, indeed, act as a bias in the choice of a profession; a youth with flabby muscles is not likely to become a blacksmith's apprentice, but what constitutes the special aptitude for the cobbler's or the baker's trade? To prove the existence of a principle of selection in these cases we should have to prove that the apprentices of these trades belong almost always to families that have no disposition to tubercular consumption,—which, unfortunately, is far from being the case.

How, then, can we know, in any given case, whether there is convergence, divergence or parallelism of causation? To interpret statistical data means nothing but to decide this question or to determine the degree of causal connectedness in the given group of phenomena, and what we want, is a practical *criterion* implied in the data themselves.

It is clear that even connected phenomena must vary, unless they have *all* their causes in common, which hardly ever happens. The regularity of their variations must, therefore, depend on the number of the causes they have in common, that is to say, on the degree of their causal connectedness. Consequently the degree of their connectedness must be inferable from the degree of regularity in their variations, and statistical interpretation must essentially consist in an *estimate of regularity*,—a task of considerable vagueness and delicacy.

When speaking of averages we tried to show that. although any sequence of values, be it regular or irregular, must have its arithmetic mean, this calculated and always calculable value cannot be used as a fair representative and convenient substitute of the data themselves, unless their discrepancies from the mean value are suffi-

ciently small and their distribution on either side of it sufficiently even to warrant the assumption that there is but one real value and that the discrepancies are due to incidental perturbations whose causes form no essential part of the enquiry. We also saw that, of these two conditions, the smallness of discrepancy (or statistical *dispersion*) is less essential than the evenness of distribution. But what is the value of such a criterion, unless "smallness" and "evenness" can be properly defined? Considering the vagueness of these terms, we might have thought that none but arbitrary or conventional definitions were possible, but by introducing the *number* of data, or the number of the series of data, as a *tertium comparationis,* statisticians have succeeded in obtaining algebraic formulas whose exactness or validity can be *ad libitum* increased by increasing the number of observations. One of these formulas gives an algebraic definition of the *"probable deviation,"* the other of the so-called *"statistical precision."* Both these values are calculable from the arithmetic mean of the given data and the number of these data, and it is clear that the larger this number of data is, the smaller must be the "probable deviation" and the greater the "statistical precision," so that the "precision" is inversely proportional to the "probable deviation," and the "probable deviation" would seem to be inversely proportional to the number of data observed, though in reality it is inversely proportional to the square root of this number of data. We cannot enter into mathematical details to prove this last proposition, but the general significance of those terms and their relation to each other is sufficiently clear to make the following intelligible:

In order to decide whether a series of data may or may not be dealt with on the presumption that it is a (slightly vitiated) manifestation of law and intelligible causation, calculate, from their number and their arithmetic mean,

the "probable error or deviation," that is to say, the deviation from the mean which is as often exceeded as not reached within this series of observations. Then calculate the average deviation directly from the (positive or negative) data. If the two values are equal or nearly so, the presumption on which the theoretical value was calculated, is correct and the phenomenon is the effect of causes which work according to the law of chances, that is to say, with theoretically absolute precision. If the theoretical value is smaller than the average of the real data, the presumption of a certain causal connectedness will still be legitimate, but the number, nature and working of the presumably common causes becomes more and more uncertain.

The third case, of the theoretical value being greater than the average of the real data, can hardly be admitted as possible. The disturbing elements which obfuscate causation lying all on the side of nature or reality and never on the side of theory, practical precision can never be greater than theoretical or ideal precision, unless special contrivances were at work, which would, of course, vitiate the whole problem.

This rule may be expressed in a more popular form, by substituting graphic representation of data for tabulated numerals. We then may say that although these data are detached points which we may multiply but cannot connect, yet if they *appear* to lie on a curve and are sufficiently numerous, we may assume that all the intermediate points not determined by observation are likely to lie on the same curve. In assuming this, we assume that each point of the curve is, somehow, determined by its two neighbors and the whole curve by any part of it. In other words: we assume the curve to be expressible by an equation and the phenomena observed to be the result of a law inferable from them.

If the points do not appear to lie on a continuous curve, and if the broken line connecting them shows no alternate deviations from some ideal medium, we may attribute to them that degree of causal connectedness which cannot be traced back to the causes themselves but only to some anterior (ancestral) phenomenon whose graphic representation shows similar ups and downs as the given one. Or we may deny their causal connexion altogether and consider the data as *results* either of "accident" or of arbitrary volition, in which case we have to show cause why they should not be discarded altogether as a meaningless plurality without any bond of union. A crystal may interest us, but hardly a heap of sand. The shape of such a heap may be very curious, so curious that it attracts notice and deserves *description,* but the more curious it is, the less likely is it to occur again and the less likely are we to discover the various causes that happened to produce it, such as the size, shape, roughness, moisture of each molecule. We may say, therefore, that *the descriptive or historic interest in a collective phenomenon is greatest where the scientific or theoretic interest is smallest.*

These three cases correspond exactly with our original classification which we will now recapitulate.

1. There are cognate phenomena *connected* by descent from at least one common cause.

2. There are collectively related phenomena which are to each other either as two (successive) generations in the causal pedigree, or as a hidden but inferrible state of things is to its manifestation, so that the latter is suggestive or *symptomatic* of the former. And there are

3. *unconnected* phenomena which not being derivable from common causes are mere *results* to us and have, through their accidental or intended similarity, an historic or descriptive rather than a scientific interest.

But there are different degrees of connectedness as there are different degrees of accidentality, so that the first as well as the third of these categories requires a subdivision. The common cause of a group of phenomena is either known *a priori* or is inferable from them. That is to say: the collective phenomenon observed is either identical with some known deduction from some known principle, or it only looks as if it might be such a deduction. In the former case we have mathematical certainty; the curve reminds us of its formula. In the latter case we have inductive approximation; the curve tempts us to infer from it some empirical formula which may or may not hold good for the non-observed part of the curve but which can be indefinitely corrected through additional data.

In like manner, there must be two classes of unconnected phenomena. We call them unconnected when we know the multiplicity of their causes but not these causes themselves. Now, this ignorance may be either partial or total. If it is partial, we are justified in adding conjectural causes to the known ones, and if it is total, we must give up the scientific enquiry and content ourselves with the descriptive or historic record of the facts observed. In the former case we may speculate on some more or less plausible analogies; the curve may suggest another curve; in the latter case we have nothing but historic exactness, the data being what they are, a series of points whose only discernible bond of union is juxtaposition and which refuse to be rounded off into fictitious continuities or laws.

We thus obtain the following five classes:

I.	a. Necessarily b. *Presumably*	*Connected*	Collective phenomena.
II.	*Indirectly Connected*		
III.	a. *Apparently* b. Absolutely	*Unconnected*	

But it is clear that in the first of these five classes, the reign of law being absolute and the law itself being known, we require no statistical data to elucidate or to prove it. And statistical enquiry is equally out of place in our fifth class where no law, at least no knowable law, reigns. The phenomena of this class are commonly called '*accidents*' and what 'accident' is in nature is, from a statistical point of view, *free will* in ethics. That every action must spring from a motive, may be conceded, but what was the cause of the motive? And in the case of many conflicting motives, what is the determining cause of our choice? Even those who cannot grasp the idea of spontaneity, must admit that human motives and biases are something infinitely more subtle and intricate than physical forces and physical causes. And if we have to admit the transcendency even of the latter by calling their effects 'accidents,' we must *a fortiori* acknowledge the transcendency of the former by calling them "free will." Neither the truism of proverbial philosophy: 'there is no accident,' nor the dogma of materialism: 'there is no freedom of will' can make this class of phenomena less incalculable to us, and incalculableness is as stubborn a fact as any other fact, and we may stumble over it, if we choose to ignore it.

By excluding, therefore, the first and the fifth category as lying outside the range of statistical interest, we obtain *three* classes of collective phenomena which being either *presumably connected* or *indirectly connected* or *apparently unconnected,* have this in common that they belong to the level of *probability* and *contingency,* where there is neither absolute certainty nor absolute incalculableness, but relative certainty and relative predictableness ranging between these two extremes. The causes and laws are not known *a priori* but inferred and induction being less certain than deduction, the future validity of these laws

and causes will depend on the degree of their inferableness. The empirical formula inferred from observed data will the more probably hold good for the non-observed part of the phenomenon or for future phenomena, the greater the number of data is, from which it was inferred. It is both general principle and concrete result and its retrospective adequacy is the exact measure of its prospective validity.

This, then, is the real sphere of statistics. Once more we find in it three distinct levels of knowledge, but these levels are now better defined and their aggregate range is more limited than we found it to be at the outset of our enquiry. We also possess a practical rule or criterion for deciding on which level any given problem may have tc be placed. But it cannot be denied and ought to be admitted by statisticians, that this rule fulfils only part of what it promises: it shows us quite clearly the boundary line which separates the uppermost level from the second, but not the one that separates the second from the third. When we know that a given phenomenon does not belong to the first level, the algebraic formula will not help us to ascertain whether and how far we may deal with the phenomenon as a symptom of something else. And here we have a permanent source of error which cannot, it seems, be stopped by any rough-and-ready contrivance and whose dangers must forever depend on our mode of viewing things at large, in other words, on our philosophy.

It would be a great error to suppose that such a criterion might be found in the quality or nature of the subject matter or in the scientific province to which the given phenomenon belongs. For the rubrics obtained by a classification of sciences according to their subject matter, far from coinciding with our three statistical levels, cross these three levels, as it were, at right angles, so that each

class of sciences covers part of the three levels and each level stretches across all classes of sciences. But although we cannot find the desired criterion in this way, we shall gain in clearness and width of horizon by making the most of this incongruence of the two classifications, that is to say, by using them as abscissas and ordinates in a table with two entries.

The simplest classification of sciences (excluding mathematics on one side and history on the other) is their division into

1. physical sciences referring to *inanimate nature,*
2. biological sciences referring to *organic life,*
3. ethics and social sciences referring to *collective or individual action.*

We see at once that the real *fundamentum divisionis* here is the degree of *consciousness,* which is nil in inanimate nature and greatest in human action. In biology we meet with nerve-power and "cell-souls" but the spontaniety of these cell-souls (if they have any) and the spontaneity of animal instinct can but slightly, if at all, interfere with the reign of law. Unknown and complicated though it is, organic nature is still a part of Nature and, *pro tanto,* subject to fixed laws. Instinctive actions are fairly calculable, as hunters know, and psychology itself, dealing as it does with much that is unconscious or half-conscious "cerebration" (such as habit and association of ideas) belongs quite as much to the second as to the third class of sciences. When Buckle tells us that the number of misdirected (and undirected) letters passing through the London Post Office does not much vary from year to year, we must consider this as a curious bit of information, but not as belonging to the natural history of *homo sapiens,* as if there were a law that makes it necessary that a certain percentage of human beings should act foolishly.

No doubt the temptation to make such a mistake is great, since whatsoever many persons have in common cannot be the result of individual caprice but must be a more or less general characteristic of human *nature*. But this depends entirely on the number of those persons and we must take a common-sense view of what is 'many' and what is "few." A folly committed by about 25 persons in a million cannot be said to be an essential part of human nature, and if this number remains the same year after year, it is because there is no reason why it should vary, as long as its causes, which (not lying in human nature) must lie in outward circumstances, remain unaltered. A sudden increase of culture and prosperity would alter these numbers at once, as can be proved by comparative statistics.

If we now arrange the subject matter of each science (or rather class of sciences) according to its calculableness or incalculableness, we shall find again within each science, our three statistical levels (with a marked preponderance of the central level).

Beginning with the most purely theoretical part of physical science, we have on the first level the whole mass of physical phenomena that are predictable or can be verified by experiment, failure being due to no theoretical flaw in the law of chances but to physical imperfections. Next come the "indirectly connected" phenomena of nature which, though known to be ruled by laws, are too complicated to be understood or predicted. They *seem* to be connected with other phenomena, but even if the connexion between sun-spots and draughts or between sun-spots and magnetic disturbances were proved, the prediction of either event often proves false and theories have then to be amended accordingly. All weather-rules are uncertain, but their uncertainty proves the value of meteorological statistics and can be lessened, if at all,

only through these statistics. More hopelessly uncertain, however, and more incalculable are the "apparently unconnected" phenomena which we find on the third level of this rubric and which constitute the "chapter of accident." We may count the flashes of lightning in a storm, the number of hailstones or of inundations in a given length of time and in a given area, but what follows from such data? If the insurance principle is to be applied in these cases, the bargain can have no greater fairness than a fair bet whose fairness consists in the equality of ignorance on both sides.

In like manner, the biological sciences have their three levels of relative certainty and accidentality. On the first we have all so-called laws of life. These laws being less known to us and their working being less regular than the working of the physical forces, the statistical method becomes indispensable: the normal length of the human life, the ratio between male and female children and other *typical* values on which life assurances and annuities are calculated are essentially statistical problems which could not be solved by any other method. On the second level of this rubric we must place the statistics referring to hygiene, to the insalubrity of trades, climates or localities, to epidemics, vaccination, insanity, to population and ethnographic peculiarities, to harvests, famines and to all other phenomena whose causal relations can only be inferred from succession or simultaneousness. In medicine and whenever the nature of the phehnomenon is as unknown to us as its causes, the *propter hoc* can only be inferred from the *post hoc:* the efficiency of a morbific cause, of a drug or a mode of treatment can be proved by no reasoning, yet may be rendered more or less plausible by statistics. In all these cases the statistical data are expected to reveal some otherwise hidden or doubtful connexion between one "indirectly connected" phenom-

enon, such as health, and some other "indirectly connected" phenomenon, such as habits, wealth, climate, occupation, marriage, mode of medical treatment, etc. And this connexion will be either reciprocal or one sided, that is to say: the two phenomena compared will be either *interdependent* or one will be *symptomatic* of the other. The third level of quasi-accidentality will here be occupied by all those highly complex phenomena whose factors are partly psychological and partly physical. Shipwrecks, conflagrations, railway "accidents" and even such oddities as Mr. Buckle's misdirected letters are almost always due to the joint action of outward circumstances and freaks of unconscious cerebration. The regularity of such phenomena being always small and even then only apparent, the fairness of the insurance bargain depends exclusively on the circumstances of each case.

In our last rubric which contains the effects of conscious volition, we seem to lose sight of nature, but when we consider that human action is either determined by reason or by taste or by moral motives, we see at once that here, too, we have sufficient material for our three levels. Logical and rational actions springing from intelligible motives, are always more or less predictable. We know beforehand that gold will be exported after a considerable rise in the rate of exchange, and we need not count the bankers who export it. But not all the phenomena of political economy are equally transparent: the laws regulating supply and demand, are theoretically as valid as any physical law, but their validity rests on the assumption that all men are not only rational but also morally neutral beings, neither unselfish nor excessively mean and greedy. And this assumption being false, we often see the doctrine of free trade and free competition practically refuted by "rings," "camorras," market con-

spiracies, corn-speculators and others who carry the principle to its utmost consequences. It is, in fact, rare that reason alone is allowed to regulate human action, but where this is the case, there can be no room for statistical enquiry. What statisticians have called *generic phenomena* are collective actions of this kind, whose causal connexion is *a priori* intelligible, and if they find a place in our diagram, it is because they never have the certainty of astronomical events (which are excluded from our scheme), and, rational though they are, we generally find traces of moral or conventional biases among their efficient causes.

The second class of conscious actions are truly *social* phenomena. They are more or less rational, but rationalness does not define them. The motive of the individual is hidden under, and modified by, that complex mass of social and traditional influences which we call fashion, conventionalism, local spirit or the spirit of the age. It is clear that such actions may, collectively, become exponents of human, national or local culture, of art, literature, industry, prosperity, and being indicative of a particular stage of development, their statistics are, not improperly, called *evolutionary*.

On the last level in this rubric we shall have to put all purely ethical or rather *moral* phenomena. Volition, though conscious, is embarrassed by a multitude of conflicting motives, judgments and desires, and the casting vote or decisive bias whether coming from free choice, caprice or innate preference, is generally incalculable. The statistics of crime and of suicides belong partly to this rubric.

All this may be summed up in the following diagram. which will be the more intelligible for not being encumbered with details and examples:

	A. *Inanimate Nature.*	B. *Animate Nature.*	C. *Conscious Volition*
I. Level of *Certainity*	1. Probabilities calculable from theoretical laws.	2. Empirical laws inferred from types and typical averages. (Biological statistics)	3. Rational action guided by Utility. ("Generic" phenomena: financial statistics, etc.)
II. Level of *Contingency*	4. Dependent and interdependent phenomena. (Cosmic and meteorological statistics).	5. Symptomatic phenomena. (Medical and ethnological statistics).	6. Evolutionary phenomena. (Social statistics).
III. Level of *Accident* and *Freedom*	7. Incalculable (because unconnected) phenomena. (Descriptive statistics.)	8. Complex phenomena. Accidents in the sense of disasters. (Chronicling statistics).	9. Action determined by moral freedom. (Statistics as historical record.)

Of course, these lines of division are no absolute boundaries. There are many phenomena, especially among those referring to what may be called *human nature,* which can be placed on more than one statistical level and in more than one scientific rubric. The statistics of drunkenness, for instance, are partly pathological and ethnographical, partly social or evolutionary. Duelling, though eminently a social or evolutionary phenomena, may also be considered as a moral one, and suicides which are essentially moral phenomena, may also be dealt with either as social or as pathological phenomena, when we have reason to suppose that their number is greatest in times of reckless competition or in certain seasons of the year.

The reason of all this is, that the term " human nature" has a double meaning. In one sense, human nature or the nature of an intelligent and moral individual is the

conscious negation of brute nature, but human nature as the complex of qualities and capacities common to most human beings, is, to a certain extent, an object of natural history and natural science. In this latter sense it will be less calculable than physical nature but more so than individual nature. The freedom of individual will, even if it were only apparent like the accidentality of "accident," would remain a practically incalculable agency. Its manifestations may be counted and registered, that is to say, many free and independent actions may prove sufficiently similar to form a collective phenomenon, but the numeral of this phenomenon can give us no clue to the recesses of the human conscience, and we shall never be able to talk of, and to reckon with, moral bias, emotion, genius, inspiration as we can talk of, and reckon with, gravity and electricity and the more reliable forces of our animal nature such as greed, hunger, or vindictiveness.

The way in which we deal with statistical problems, and the criteria we use in assigning to each given problem its level and its rubric, must greatly depend on our general philosophy or "world-view." Our diagram helps us to no algebraic formula that were applicable throughout its nine categories, but it has the advantage of bringing order and clearness into the great mass of materials to which the statistical method of enquiry can be applied. It shows us the limits of statistical competency which lie not so much on the four borders of our diagram as near its four corners, for, while the sphere of scientific statistics must lie, in one sense, between certainty and accident, it lies, in another sense, between physical forces and conscious motives. Both physical causes and conscious motives may belong to it, but only when the physical causes are sufficiently numerous and their action sufficiently complicated not to be calculable and the conscious motives are sufficiently common not to be incalculable.

Regularity of action being then disguised by the complexity of the phenomenon, the physical causes will have a *subjective* uncertainty of action as great as the objective uncertainty of biological phenomena; and individual spontaneity being disguised by social sameness, the conscious motives will have a subjective certainty apparently as great as the objective certainty of physical phenomena.

On our first level, but more particularly in its first and third category, statistics can never be more than an auxiliary for elucidating otherwise intelligible laws. On our last level, but more particularly in its first and third category, statistics may count and register, describe and chronicle, but must beware of arguing. Argumentative statistics being superfluous in our first and third category, become more or less impossible in our seventh and ninth category, and only in the five remaining categories formed by the intersection of the two central rubrics, statistics can claim the rights and honors of an *independent science.*

Notwithstanding all this, there is a two-fold tendency among statisticians to contract this legitimate domain of theirs, the one acting horizontally from right to left, the other vertically from below upwards. The materialist who denies human freedom and sees in life nothing but a physico-chemical process, will be prone to transfer statistical data from the third vertical rubric into the second and from the second into the first, and the mystic who has this in common with the materialist that he denies accidentality, will be prone to promote phenomena from the lower levels to the highest level, which is the level of the reign of law. The former tendency can only last as long as the reign of materialism lasts, but the latter tendency is far more permanent because it is common to the rationalist, the believer and the mystic, who all agree in repudiating accident and differ only in the

name of its substitute, which is fixed law to the rationalist, God or Providence to the believer, occult powers, fate, "manifest destiny" to the mystic. The error here contemplated does not lie in any of these creeds but in the tendency, common to them all, of rejecting a practically useful and indispensable term on the ground of a theoretical truism which is admitted by all who use the term 'accident' in its subjective meaning.

The first tendency, when acting alone, induces statisticians to ascribe to unconscious agencies what is due to conscious motives; the latter, when acting alone, induces them to ascribe to causal connexion what is due to outward similarity. The former effaces moral responsibility, the latter scientific honesty; the former destroys consciousness, the latter hypostasises the unconscious; the former makes statistics *nihilistic,* the latter *cabalistic*..

Many weather-rules become cabalistic through excessive specialization. We are apt to become cabalistic when laying too much stress on the statistics of street accidents, of twin-births, of psychological freaks, of misprints or other oddities occurring, with apparent and transitory regularity, with certain limits of time and space. All talk about good luck and bad luck, if founded on ever so many statistical data, is essentially cabalistic, and cabalistic is the statesman who recognizes the *vox Dei* in the roars of the mob or in the whims of public opinion. A political cry, provided it gratifies the lower aspirations of the people, will soon be joined in by a large number of persons: it may then become an irresistible force, but can never be a sign or proof of "manifest destiny."

If, on the other hand, the theory were propounded (and it has been propounded) that sun-spots and human prosperity are concomitant phenomena, we should have a typical example of the other kind of statistical error

which we have called the materialistic error. Many conclusions drawn from phrenological data (especially in criminal statistics) belong to this class of fallacies, and the same must be said of those fanciful statistics of genius or other forms of human greatness, when pretending to prove the dependence of such phenomena on topographical data, on food, on latitude, on isothermal zones.

Crimes are, and ought to be considered as, conscious actions. As such they belong to our third rubric and more particularly to its third level, which is the level of moral freedom. They are essentially individual, and outward similarity does not always suffice to make them countable as a collective phenomenon. A hungry man may steal eatables, a a banker's clerk having his mother, his sisters and a family of children to maintain, may defraud his rich employer, but what do we gain by counting and booking such crimes? They will occur occasionally, as long as human frailty lasts, and they will occur in periods of prosperity as well as in periods of commercial depression. The temptation here is as intelligible as the rational motive in those actions which constitute "generic" phenomena, but these crimes are not generic phenomena for all that, for, although their motive may be as intelligible as in the case of innocent expediency, its efficacy (or power of inducing action) can never be as certain. A thousand persons may feel the temptation, but we cannot tell how many or whether any will act on it.

We admit, however, that a great number of crimes, such as fraudulent bankruptcy, professional robbery and murder, furnish items for collective phenomena. Being acts of warfare which the criminal wages against society, they are, to a certain extent, induced by society, and these statistics then become social or evolutionary phenomena belonging to the second level of our third rubric.

We must further admit that when the purely animal factor of crime predominates over its mental or moral

factor, as is the case in all crimes of violence and brutality, such crimes may furnish data indicative of certain phrenological types or of pathological states such as insanity. A certain part of criminal statistics may, consequently, belong to our second rubric which, embracing all biological phenomena, is the battlefield of mental and physical agencies. Here our jury-men find their store of "extenuating circumstances" which we are far from grudging, but it is clear that one may go too far in this direction and that the more we shift human action from its proper category towards that of physical events, the smaller will be our appreciation of moral responsibility.

The materialistic philosophy which has induced this tendency, may claim the merit of philosophic consistency. But what shall we say of those shallow thinkers and law-givers who deal with crime as if it were nothing but a form of viciousness repressible by fear. The Swiss people who have just voted for the re-establishment of the pain of death and for the publicity of executions, have shown thereby that they are firm believers in the deterrent effects of capital punishment. They had abolished it, because there was a time when their prisons were all but empty and when crimes of any kind were extremely rare. The last seven years having shown a startling increase of crime and especially of murder, the Swiss were not wrong in dealing with this phenomena in its social and evolutionary sense. It would have been rational to enquire whether the presence of 8,000 Italian workmen on the St. Gotthard line or the nihilistic spirit emanating from the Swiss schools and universities had anything to do with it. But to treat it at once as a purely psychological phenomena caused by the disappearance of a penalty which had for generations proved superfluous, was a bit of crude reasoning which looks logical but is absurd.

The two tendencies mentioned above, though apparently due to two opposite habits of thought, are often at

work together, and the one acting from below upwards while the other acts from right to left (in our diagram), the tendency resulting from their joint action must be in the direction of the diagonal, so that all the influences apt to vitiate statistical reasoning may be summed up in a general tendency to consider all collective phenomena as effects of intelligible causes and as causes of probable effects. There are economists, for instance, who confidently believe that we are now entering on an area of prosperity which is to last five years, and who have no other ground for believing this than the equal length of the late area of commercial depression. This is both cabalistic and materialistic, because it implies that these alternations and their respective lengths (for which we are mainly responsible ourselves), are regulated by mystic laws and by powers over which we have no control. Mr. Buckle tried hard to prove statistically the regularity and calculableness not only of events but of human actions. Though writing while Hartman was still in his teens and some years before the Darwinian doctrine of inheritance had become a common creed, he was, unconsciously, a true worshipper of the Unconscious. But what has science gained by this new worship? To have discovered the mystic vestiges of an occult power combining the functions of Registrar-General with those of a universal tyrant, may be a merit but ought not to be the boast of the rationalist. Yet, if statistics have ever become cabalistic, it was due to this peculiar form of rationalism called materialism, which cannot try to be deep without lapsing into mysticism.

III. Little remains to be said about the *prospective interpretation* of statistical data or the *estimate of probabilities*. We know that the certainty of the reoccurrence of a phenomenon is, theoretically, as great as the relative certainty with which it can be derived from its causes.

Its probability may be called its prospective causality, just as its causal connexion may be called its retrospective probability: any error of diagnosis will induce error of prognostication. Much, therefore, of what can be said about probability, is implied in what has been said already about causal connectedness or inferribleness of causes.

It is important to remember that the term "chance" which is often used in the sense of accidentality, is, philosophically, the reverse of accident, since it implies relative calculableness, while accident is absolute incalculableness. There is a *law of chances,* but there can be no law of accident, so that the calculus of probabilities will be most applicable on the first level, less so (but still applicable) on the second, least on the third,—not because there is any flaw in the theory of chances or any unsurmountable algebraic difficulty, but because the validity of chances rests on assumptions which are truest on the first and least true or altogether impossible in the last level.

The algebraic measure of a chance is a fraction whose numerator is the number of favorable cases (or favorable possibilities) and whose denominator is the number of all the possible cases. But we have seen that all statistical data, though apparently numbers, are ratios, percentages and therefore fractions whose numerator represents the number of observed facts (or these calculated averages) and whose denominator is the sum of all the observed cases of non-occurrence as well as of occurrence. Thus every statistical ratio might be supposed to represent a fraction of probability, and, to a certain extent, this is the case, but it is only true when the possible cases represented by the denominator have all the same degree of ideal possibility, that is to say, when they are absolutely homogeneous. In the game of dice, the probability of scoring ten is 3/36 or 1/12, the favorable cases being three, the possible ones altogether 36. But if in

times of an epidemic, three persons have died in an establishment of 36, we cannot say that out of any other group of 36 persons, three are likely to die. The fraction 3/36 or 1/12 does not represent the probability of death in the sense in which it represents the probability of scoring 10 with two dice, because the possibility of death is by no means the same in thirty-six persons of different age and constitution, while the possibility of the thirty-six combinations in the game of dice are absolutely equal. Of course, we can make our denominator more homogeneous by making many series of observations and calculating the mean value or, which comes to the same, by enlarging the denominator and reducing the new fraction to the former unit. If in a thousand groups of 36 persons the arithmetic mean of the thousand death-rates were 3000/36000, this fraction, which is again equal to 1/12, would indeed represent the probability of death for the survivors, but even then its validity would be less great than the probability of scoring ten with two dice. It would be, at best, but a probability belonging to the second level, whereas the chances in a game of dice belong to the first. By multiplying our observations we have eliminated only that factor of variableness which belonged to the difference of validity and morbidity of the population, but not the other factor which belongs to the increasing or decreasing virulence of the epidemic and which obviously cannot be eliminated at all.

It is different with those vital statistics which refer to general mortality under presumably normal and permanent conditions. Here every statistical ratio will be a fairly valid expression of probability, and life insurance companies, provided they revise and correct their tables from time to time, have all the materials for offering fair terms to their associates. That perfect fairness can only be obtained through *mutual* insurance, is a matter of course.

The further we descend from this level of typical phenomena, the less fair becomes the insurance compact, even when its *bona fide* object is not undue gain but to provide against dangers over which we have no control or to secure indemnity where immunity is out of the question. Such dangers are disease, old age, widowhood, fire, shipwreck, floods, hailstorms, railway accidents, etc., a variety of emergencies which being partly meteorological, partly biological, partly psychological and generally of a mixed nature, can hardly be dealt with in a uniform manner. How can we calculate the chances of floods or hailstorms? We have before us a *mare magnum* of possibilities whose probabilities cannot be surveyed from the *terra firma* of experience. Averages have no meaning here, the dispersion and irregularity of the observed data being far too great to justify the belief in anything like a typical mean. The valuation of chances must, in such cases, rest on the observed maxima, these maxima being the limits *not likely* to be exceeded. This is, no doubt, an injustice to the policy-holder, but it is made good by the comparative rareness of the occurrence, that is to say, by the relative smallness of the danger even at its highest valuation and by the corresponding smallness of the premium. The compact, then, though scientifically false and hopelessly, incorrigibly false, becomes practically fair enough.

The same may be said of railway accidents. They are highly complex phenomena dependent on such a variety of physical, psychological and even ethical conditions that even careful judicial enquiries often fail to ascertain the degree of imputableness belonging to each of the possible causes. To speak of a calculable probability of such accidents seems absurd, and yet there are insurances, not against these incalculable events but against their possible and doubly incalculable consequences such as loss

of limb or life. Here, too, the compact can never be strictly fair, but it becomes unobjectionable and even useful, when the danger is small and the number of policies so large that even the smallest premium suffices to make the insurance remunerative. The probability, in this case, is not a ratio but an endless series of ratios, or graphically, not a line or a curve, but an ill-defined zone of possibilities: it is a probability of the third order befitting the nature of the phenomena which belong to our third level of statistical enquiry.

The chances of a lottery and those of most games are theoretically calculable and obviously belong to our first level, but, as a matter of fact, the hopes of gamblers are more frequently disappointed than fulfilled, and this discrepancy between theory and reality is due either to unfairness of terms or to the *necessarily* false assumption on which even *bona fide* transactions of this kind rest, viz., that the calculated possibilities are all equally possible. They ought to be, but never are and never can be, quite equally possible. Dice, for instance, are never absolutely perfect in shape or absolutely homogeneous in molecular structure: their centre of gravity rarely, if ever, coincides with their geometrical centre. The imperfections of lottery-marks, lottery-wheels, playing-cards and other gambling utensils are altogether innumerable and a microscopic defect may act and must act as a permanent bias. The fairness of any game of chance, therefore, does not lie in the assumed equality of the calculated possibilities, but in both gamblers' common ignorance of their irregularities. That the ratio between stake and prize must be equal to the calculated ratio of chances, is a matter of course and does not concern us here, but the impossibility of perfection in tools renders it necessary that these tools should be used in turns by each party, and

where no tools are requisite, as in a bet, the bargain can only be fair when both parties are equally ignorant of the final issue and when its chances are either incalculable or have not been calculated by either: the difference of strength in their convictions is then expressed by a difference of wagers, and *subjective probability* takes the place of objective or theoretical probability.

Most social phenomena having to be treated according to the rules of the second level, will yield probabilities of the second order, and it is a greater mistake to place them too high than to place them too low. But we never hear of the latter and lesser mistake, the upward tendency being, as we have seen, universal throughout the domain of statistics. The very common belief in regular cycles of commercial and industrial prosperity is proof of this tendency. Surely if social prosperity were a phenomenon representable by a curve with regular undulations, we might boast of having solved the greatest of all social problems. The equation of that curve could only contain variables belonging to physical nature and would be a law of nature empirically inferred as a Turk's illness is the inferred will of Allah. And what is this belief founded on? M. Lefevre, the President of the British Statistical Society, informs us that every tenth year is a year of greatest depression, that this has been so during the last thirty years and that Prof. Jevons has succeeded in tracing the same periodically as far back as the beginning of the eighteenth century.* The readiness with which such generalizations are always accepted by the general public, is hardly intelligible when we consider the extreme slenderness of their numerical foundation. What do we know about the commercial statistics of the year 1700, what of the innumerable whims of trades and moods of industry in those days? Most likely the periods

* The London Times of Nov. 23, 1878, in a leader, and *The Times* of Dec. 21, 1878, in a Letter to the Editor.

of prosperity were never longer than ten years (man cannot bear prosperity very long), but who can tell us how often they were shorter? The belief in a decennial or any other cycle of this kind seems altogether irrational, for some at least among the many forces that regulate the growth and flow of capital, are purely psychological and moral. Greed and indifference, caution and courage, thrift and prodigality can coëxist in many different proportions, succeed each other at many different rates, enhance or impair each other in many different degrees. What right have we even to look out for law or fixed periodicity in financial phenomena as if they were comets or eclipses? And do not even comets defy our algebra? All we know from experience is that, sooner or later, human greed, which seems greater than human judgment, gets choked through over-production, so that, sooner or later, every rise must be followed by a depression. Considering also that, on the whole, the lower motives are stronger than the higher ones, we may go so far as to say that the periods of human prosperity are likely, in the long run, to be shorter than those of depression, but it would be intellectually unbearable and morally dangerous, if anything like a fixed and calculable law were supposed to regulate these phenomena.

We grant that solar physics may affect our planet. When we are told that harvests are caused by sun-spots, we have no theoretical difficulty in admitting the connexion, provided we have statistical data to prove the constant coincidence of the two cycles. But the more the psychological factor in any collective phenomenon predominates over the physical factor, the weaker and the less significant will be the legitimate inferences from its statistics. And when besides the physical and the psychological factors, a moral factor has been at work, our statistics lose still more of their dogmatic and prognostic suggestiveness.

Such are the statistics of crime, for instance. Unlike the phenomena of political economy, a crime need not necessarily be considered as a social phenomena, since it belongs to private rather than to public life, and still more private seems to be the nature of suicide. We may treat suicides as symptomatic and as evolutionary phenomena; we may classify them according to the seasons in which they occur, according to the moods of execution or according to their motives. But if we do not make these distinctions, if we consider suicide merely as the action of a morbidly biassed human will, without any reference to the nature of the bias, we shall not be much the wiser for counting the occurrences. The phenomenon then ceases to be a connected phenomenon, and its probability ceases to be calculable. The probability of a crime or the probability that any given individual will commit suicide, is not more but a good deal less calculable than the occurrence of a flash of lightning or of a hailstorm or of a railway accident. Neither the yearly averages of suicides nor the budget of the guillotine (as Quételet calls the statistics of bloody crimes) imply any oracular hints concerning the future. We are not bound to decide (by lot or otherwise) who and how many of us are to commit suicide or murder to make true the averages of the past, nor need we fear the fate of Oedipus who fulfilled the oracle by trying to evade it. These averages are, in fact pure fictions and refer to independent facts as unconnected with each other and as different from each other as the novels of Sir Walter Scott, which we may count and read, but which cannot be summed up in an average novel.

Statisticians, when translating their ratios and averages into probabilities, feel constantly tempted to ask whether there is not, in collective individual action, some unknown compelling force acting on society as if it were

a unit and unknown to each individual. But this question implies that the data and averages at our disposal are more or less constant or typical values, whereas they represent, in each case, but infinitesimal sections of an endless line of evolution; of a line, therefore, of whose straightness or curvature we know next to nothing. Who can tell us, whether human development is necessarily and always progressive, or whether its final decay does not form part of it, as it does in individual life? And if we can shorten our life through carelessness or vice, why should we not be able to shorten the evolutionary phase of social life and to hasten the process of social decay, through the sins or omissions of each of us? We do not deny the reign of law in organic life by asserting that we are in a great measure responsible for disease and premature death. Nor do we imply that causality ceases to be valid in ethics and in history, when we assert that we are, each of us, in a great measure responsible for the woes of mankind and the miseries of life. We cannot interpret collective phenomena and their statistics either rationally or profitably, unless we emancipate ourselves from all materialistic and cabalistic dogmatism and take a common-sense view of individual freedom, The human will, though essentially free, is of course not absolutely free. It can assert this freedom only by reacting, successfully or unsuccessfully, against outward circumstances and these outward circumstances may lie within ourselves, though remaining outward with regard to our innermost self. But *of these reactions we are not always conscious, and it is the object of social and biological statistics to bring them home to us, to make us conscious,* not so much of the powers and forces acting on us, as *of our reaction against them.*

These outward influences being numberless, we cannot, while living in the flesh, emancipate ourselves from their

collective action. But, taken singly, they are variable and there is no *a priori* reason why any one of them should not be made to disappear or be reduced to an imperceptible minimum in the course of time and through our own efforts. Evolutionary statistics mark a state of society at a given moment or in a given place. Such a state we call fashion, public opinion, spirit of the age, and the spirit unquestionably prejudges the future by guiding the will and the actions of many persons. But many are not all, and not always are they in the majority, though they often appear as such by occupying the surface of social life. As a matter of fact, there are always some people, no matter how many or how few, who are either " in advance of their age " or in some other sense opposed to the reigning spirit. Whence comes this advance, whence this opposition?

If mind were matter, evolution would be a mechanical process of absolute necessity and absolute objectivity. But materialism, which is a good working hypothesis is physical science, is unable to explain social phenomena and utterly breaks down when applied to aesthetic or to moral phenomena. The annals of history will never be to the historian what ephemerides are to the astronomer, and to treat history as if it were a chapter of natural history, is to mislead the human intellect by flattering its pride and to *shirk* responsibility by throwing the weight of our sins and omissions upon the innocent shoulders of Nature. When we happen to be among the lean kine. the best thing we can do is to feed and fatten them and not to wait for the coming of the fat ones, whether they be seven as in the days of Moses or ten as the modern cabala pretends. In other words: when the cumulative effects of universal greed and dishonesty manifest themselves in form of distress and depression of trade, it is more rational to mend our ways and cause others to mend

theirs than to look out for sun-spots and cycles. It will then be found to be within our power to destroy the regularity of these cycles and, with it, their statistical significance.

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